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Shaping scientific excellence in agricultural research

Joanna Chataway

Development Policy and Practice Group
The Open University
Walton Hall, Milton Keynes MK7 6AA, UK
Fax: +44 01908 654825
E-mail: j.c.chataway@open.ac.uk
and
ESRC Innogen Centre
Technology Faculty
Development Policy and Practice Group
The Open University
Walton Hall, Milton Keynes MK7 6AA, UK

James Smith*

Centre of African Studies
The University of Edinburgh
21 George Square, Edinburgh EH8 9LD, UK
Fax: +44 0131 650 6535
E-mail: james.smith@ed.ac.uk
and
ESRC Innogen Centre
Institute for the Study of Science, Technology and Innovation (ISSTI)
The University of Edinburgh
Old Surgeons' Hall
High School Yards
Edinburgh EH1 1LZ, UK
*Corresponding author

David Wield

Development Policy and Practice Group
The Open University
Walton Hall, Milton Keynes MK7 6AA, UK
E-mail: d.v.wield@open.ac.uk
and
ESRC Innogen Centre
Technology Faculty
Development Policy and Practice Group
The Open University
Walton Hall, Milton Keynes MK7 6AA, UK

Abstract: Science and technology – and particularly biotechnology – is increasingly central to development agendas in Africa and elsewhere. Implicit within the centrality of science and technology lie a set of policy issues regarding how best to shape contextually appropriate, innovative and sustainable science and technological products in, with and for developing countries. The work of the Consultative Group on International Agricultural Research (CGIAR) is a case in point and we draw our empirical material from the evolution of two biotech bovine vaccine development programmes housed in a CG Centre. In the paper, we seek to show that broadening our understanding of scientific ‘excellence’ can lead to more innovative, systemic research that may produce more appropriate technological solutions. We believe this has key implications for science policy, development policy and the practice of science for development itself.

Keywords: science policy; research and development; vaccines; Consultative Group on International Agricultural Research; CGIAR; scientific excellence; research for development.

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Biographical notes: Joanna Chataway is a Professor of Biotechnology and Development at the Open University. Her research and consultancy experience includes work on science and technology capacity building, North-South Public Private Partnerships (PPPs), innovation and development. Chataway has carried out several studies looking at these issues in relation to agricultural and health related biotechnology and has also researched risk regulation, perception and management of risk of biotechnology.

James Smith is a Lecturer in African Studies at the University of Edinburgh, a Principal Investigator in Innogen and a Visiting Researcher at the Open University. Previously, he was based at the University of the Witwatersrand in South Africa and at Oxfam. His research interests includes science policy, innovation and development, and development theory. He is currently working on a project looking at innovation partnerships for genomics for development in Africa, Asia and Latin America.

Dave Wield is co-Director of the ESRC Innogen Research Centre and a Professor of Innovation and Development, based at the Open University. His research interests include the following: regulation of science and technology; institutional support for innovation, including science and technology parks and organisational learning; technological capability building in conditions of transition and development. His most recent book is: *Followers to Leaders: Innovation Management in Newly Industrialising Countries*, Routledge (with N. Forbes).

1 Introduction

The role of science and technology in development is back on the agenda. High level reports make clear that S&T research, particularly biotechnology, has an important contribution to make to development – both because it is key to addressing problems in health, water supply, energy and food security, and also because it “unlocks the potential

of innovation and technology to accelerate economic growth” (Commission for Africa, 2005). As the Africa Commission report asserts, “specific action for strengthening science, engineering and technology capacity is an imperative for Africa” (Commission for Africa, 2005). It is also clear that the way in which scientific research is undertaken has changed – new technologies and research methods have emerged, new areas of interest have arisen, new disciplines have developed. What is less clear is whether systems for evaluation of research have adequately evolved alongside the broader changes in the field to ensure the level of research excellence – and the type of research excellence – that is necessary for development.

The traditional method of evaluating research excellence has been through peer review of journal articles. Peer review publications are an important and probably necessary component of evaluation, however using them as primary measures of performance may give inappropriate signals to researchers wanting to address complex and necessarily interdisciplinary problems of agriculture and poverty. This method gives very limited incentive for researchers to respond to the need to promote development partnerships and networks and to engage in the messy interdisciplinary work that this often entails. Sensitivity to local context and changes in the way research organisations are structured may also be relevant. This points to questions of the definition of research excellence and how it is measured. More broadly, however, it also underlines questions of institutional design, governance, and funding mechanisms. These questions of research excellence are taken up in this paper.

This paper explores the ways in which notions of what is research ‘excellence’ shapes knowledge production, technological innovation, development practice and ultimately the poverty alleviation agenda. The paper examines recent research and development with regard to the development of vaccines for bovine *trypanosomiasis* and *theileriosis* primarily at the International Livestock Research Institute (ILRI), a member of the Consultative Group on International Agricultural Research (CGIAR). The CGIAR is an alliance of countries, organisations and foundations that support 15 international agricultural centres each with a remit to conduct research on issues relevant to developing country agriculture. The CGIAR represents the single largest public goods investment in pro-poor international agricultural research in the world and as such plays an important role in shaping agricultural ‘research for development’ at a whole range of levels and in many different ways.

The paper maps out differing approaches to the management of bovine *trypanosomiasis*, a protozoa that causes significant livestock and economic losses amongst non-indigenous cattle across sub-Saharan Africa. One of these approaches, based at the predecessor to ILRI, focused on the production of a vaccine, whilst another was a simpler vector management technique. The vaccine approach was characterised by a very narrow, institutional focus driven by a science-led agenda. By contrast the vector management technique was developed outside of an institution. After work towards the creation of a *trypanosomiasis* vaccine halted in the mid 1990s work continued towards the development of a *theileriosis* vaccine at ILRI. *Theileriosis* also causes high mortalities in breeds nonindigenous to the endemic areas of eastern, central, and parts of southern Africa. The organisation and rationale behind this recombinant vaccine endeavour was quite different from the *trypanosomiasis* approach. These case studies speak to several important questions about how we conceptualise research ‘excellence’, particularly in a developing country context, and how ‘excellence’ does more than label science as a successful or failure but also seeks to prescribe how research is conducted,

organisationally and conceptually. This analysis will seek to uncover how particular conceptualisations of excellence shapes decision-making around funding, donor activities and research endeavour, with concrete results for *trypanosomiasis* and ECF management. The paper is split into four sections. The next section sketches a little of the history of the CGIAR and in particular ILRI and its predecessors and the forces that have shaped policy and research in the institute. Sections 3 and 4 will discuss the institutional histories of *trypanosomiasis* and *theileriosis* research in East Africa. Section 5 concludes by discussing some implications of these case studies for science policy in developing countries, institutional organisation and notions of research 'excellence'.

2 Institutional context

The CGIAR system was born in 1971 with the objective of focusing on 'international agricultural research and related activities', thereby contributing 'to improved sustainable food production in LDC's such that the nutritional level and general economic well-being of their low-income peoples are improved' (CGIAR 1992 cited in Clark, 1992). The CGIAR came to comprise 15 centres located in different parts of the developing world, a central secretariat and a Technical Advisory Committee (TAC). An outgrowth of the optimism surrounding the Green Revolutions, the CGIAR it was hoped, might produce excellent science focusing on central problems and constraints associated with agriculture in developing countries (Anderson *et al.*, 1991).

Autonomous research centres coalesced under the CGIAR umbrella would produce excellent applied science to be passed on to national agricultural systems. The CGIAR secretariat and the Technical Advisory Committee which serviced all the centres would fundraise and offer advice on the direction on work, but would not engage directly in the management of individual centres. The belief was that independent centres giving high levels of flexibility to teams of scientists were the best vehicle for addressing poverty (Baum, 1986). CGIAR centres have had numerous achievements but nothing on the scale of the early Green Revolution technologies. It is also now a widely held belief that the centres have been much more successful in addressing the problems of middle income farmers with access to resources and inputs than meeting the needs of the poorest small scale farmers. Driven by a range of different pressures for change the CGIAR system is in the throes of change designed to enable it to meet the needs of the poor in the developing world.

The CGIAR's vision today is undergoing a period of restructuring:

"The CGIAR system is undergoing redesign to refocus its efforts in a rapidly changing world and to make it more effective and visible in the future. This has led to a restructuring of the management of the system, a new focus on raising external funds from unconventional sources and a new strategy for the future. The CGIAR vision is based on a two pronged approach: reducing poverty and improving food security." (CIP, 2004)

One centre has explicitly aligned itself with the Millennium Development Goals (CIP, 2004), so that it will judge its own performance in relation to progress towards the MDGs. Another centre, the International Rice Research Institute is currently reworking its strategy along similar lines. Whilst other centres remain more closely linked to research targets, all now see themselves at least in theory as very much part of a complex

development project rather than producers of science to be applied by others. Changes in external orientation are matched by quite radical shifts in internal organisation of the CGIAR system. For instance the CGIAR secretariat and the newly formed Science Council, which has replaced the Technical Advisory Council, exercise more authority over the Centres and there is much more emphasis on system-wide activities, implying collaboration between centres, than there was in the past. This reflects the perceived need for much more networking and partnership based activity. There is now a widely held belief that whilst science and technology remain key to alleviating rural poverty, responding to the needs of the poorest, requires intense collaborative and interdisciplinary work.

The journey from applied laboratory based research centres to integrated development actors has been at times fraught and is certainly not complete. Replete with contradictions and difficulties the work and focus of the CGIAR centres remains controversial and increasingly difficult to conceptualise. One of the pivotal areas of current debate is about how the work of the centres should be evaluated. How is work judged to be excellent or otherwise? In order to understand the debate it is important to go back and consider a little more of the CGIAR history. In this paper, we briefly explore that history focusing on vaccines work at ILRI.

3 The institutional evolution of ILRI

ILRI was officially formed in 1994, following the ‘disestablishment’ of two other CGIAR centres – the International Laboratory for Research on Animal Diseases (ILRAD) and the International Livestock Centre for Africa (ILCA). In 1973, ILRAD had resulted from a Memorandum of Agreement was signed between the Government of Kenya and the Rockefeller Foundation (acting on behalf of the CGIAR) for the establishment of ILRAD, as an international and autonomous, non-profit organisation. According to this agreement:

“(t)he purpose of the Laboratory will be to serve as a world centre for research on ways and means of conquering, as quickly as possible, major animal diseases which seriously limit livestock industries in Africa and in many other parts of the world. The Laboratory will concentrate initially on intensive research concerning the immunological and related aspects of controlling *trypanosomiasis* and *theileriosis* (mainly East Coast Fever). It may, however, eventually extend its research to other serious animal disease problems for which its facilities and expertise are appropriate...In carrying forward its program, the Laboratory will develop close linkages with governmental and regional organisations undertaking research on the same or related disease problems.” (ILRAD, 1973)

ILRAD was, therefore, set up as a laboratory-based scientific research institute with a global mandate to develop immunological solutions (mainly vaccines) to *theileriosis* and *trypanosomiasis*. In practice, as a result of the distribution and effects of the diseases, ILRAD would focus on Africa. The explicit focus on immunological solutions; vaccines, and latterly particularly sub-unit vaccines, represented a conscious institutional decision to focus attention on one possible solution rather than other potential solutions such as vector entrapment, or spraying and dipping using acaracides in the case of East Coast fever and trypanocides in the case of *trypanosomiasis* (Homewood *et al.*, 2006). There

were, and remain, issues, deficiencies and negative impacts with all alternative solutions but it is clear from interviews with ex-ILRI staff that an immunological solution would be the scientifically most ‘elegant’ solution.

ILRAD initially focused on developing a sound epidemiological and immunological understanding of the two diseases. Early achievements in *trypanosomiasis*-related research included *in vitro* propagation of one of the main trypanosome species of interest, and the discovery of a limited number of metacyclic antigens in this species. In the field of *theileriosis*, investigations of immune responses to *Theileria* infection had taken place, along with the development of techniques to culture *T. parva* strains. By the early 1980s, a vaccine against East Coast Fever (ECF) was expected within the next five years. An ‘immunological solution’ against trypanosomosis, however, was proving elusive – the complexity of antigenic variation amongst trypanosomes was becoming increasingly apparent (ILRAD, 1982).

The first systemic review of ILRAD recognised this initial work as fundamental but called for a reorganisation into ‘projects’ that would encourage horizontal integration and a more multidisciplinary approach. Consequently, following the 1980 review *Theileriosis* research was divided into three project areas. The first built on established East Coast Fever epidemiology research, specifically disease control through the ‘infection and treatment’-method of immunisation. The other two *theileriosis* projects focused on developing vaccines based on antigens from *Theileria* sporozoites (form of parasite transmitted from infected ticks to cattle) on the one hand, and *Theileria* schizonts (form of parasite that develops in the cells of infected cattle) on the other (ILRAD, 1983).

Trypanosomosis research was also divided into three project areas. Again, the first focused on epidemiology. One component of this project concentrated on the identification and characterisation of trypanosomes, which fed into vaccine development. Another component included the collaborative project with ILCA and the International Centre for Insect Physiology and Ecology (ICIPE) on the productivity of trypanotolerant livestock – the Trypanotolerance Network. The second trypanosomosis project investigated methods to control the parasites, and the third at improving the immune responses of infected animals. Each project area was further divided into several ‘sub-projects’ (ILRAD, 1983). These initial attempts at organisational restructuring were a move away from the clearly linear ‘Mode 1’ characterisation of ILRAD’s early years. As these projects became more complex and involve more stakeholders, evaluating them on the basis of purely ‘excellent’ science became increasingly inadequate.

4 *Trypanosomiasis* research and development

Trypanosomiasis is caused by unicellular protozoan parasites, termed trypanosomes, which propagate in the blood and tissue fluids of their hosts. Pathogenic species of *Trypanosoma* occur in many parts of the developing world and infect amongst others, humans, livestock and water buffalo. The susceptibility of host species differs – the disease can be either acute or chronic. *Trypanosomiasis* is frequently fatal in highly susceptible animals (such as ‘Zebu’ cattle and some exotic breeds), while in more resistant ones the disease results in decreased productivity. Symptoms of *trypanosomiasis* include sporadic periods of fever, wasting, enlargement of lymph nodes, anaemia, infertility and immune dysfunction, and tsetse flies are the main vectors.

Trypanosomes assume different morphologies during their lifecycles in hosts and vectors (tsetse flies). Once in the bloodstream, the trypanosomes begin to divide, and trigger an immune response from the host – antibodies are produced against the ‘antigen face’ of the parasites. The trypanosomes have developed a survival strategy to avoid destruction by the host immune system. Trypanosome infection occurs as successive waves of parasites (known as parasitaemic waves) appearing in the blood and tissue spaces of the host. The host mounts an immune response to the repertoire of antigens present in each wave. By the time this immune response has occurred, some trypanosomes have altered their ‘antigenic faces’ and these will initiate the next wave of trypanosome infection, for which the host immune system is not prepared.

For decades, *trypanosomiasis* control has been attempted mainly through two routes – vector (tsetse fly) control and trypanocide drugs. The former has involved a range of approaches, from tsetse habitat clearings and the use of impregnated traps, to the widespread application of insecticides and the use of the sterile male technique. Indeed in the early to mid 1980s according to one authoritative source ‘the days of tsetse seemed numbered’ (Torr *et al.*, 2005). Large scale spraying at ground and aerial levels had all but eliminated tsetse from large areas of east, west and southern parts of Africa that had previously been infested. These and other promising vector control methods began to fail, however, due to changing research policy positions, donor funding priorities, and increasing pest resistance.

This was the context in which ILRAD embarked on a biological solution to the *trypanosomiasis* problem in the early 1980s, namely the discovery of a vaccine. But as outlined above the development of such a vaccine was hampered due to antigenic variation and vaccine research efforts effectively ended at ILRI approximately five years ago. The focus of *trypanosomiasis* research at ILRI has since shifted to the genetic characterisation of trypanotolerant cattle.

5 *Theileriosis* vaccine development

Theileriosis refers to a complex of diseases caused by protozoan parasites from the genus *Theileria*. These parasites invade and propagate in the cells of the immune and haematopoietic (blood cell producing) systems of their hosts, mainly cattle. As in the case of *trypanosomiasis*, susceptibility to the disease differs amongst breeds – in highly susceptible, imported and more productive breeds, the disease is acute and frequently fatal (within three to four weeks of infection). Even in more resistant breeds, lower productivity follows recovery from an infection. Hence *theileriosis* is a real development problem. In East and Central Africa the most important species is *Theileria parva*, which is transmitted by tick, and which causes East Coast Fever (ECF) in cattle. The parasite is closely related to the causative agent of malaria in humans.

Similar to trypanosomes, the *T. parva* parasite exists in different morphological states during the course of infection. Control of *theileriosis* has relied on the application of acaracides, chemical agents, against ticks. In high-risk areas, cattle have been sprayed with, or dipped in, acaracides on a frequent basis. However, this is expensive, and tick populations have been shown to develop resistance to available chemicals. Pasture management has also proved to be effective, but small-scale livestock keepers often lack the resources required to implement this. The most widely used approach remains the ‘Infection and Treatment Method’ (ITM) of immunisation, described below.

The prospects of a vaccine against *theileriosis* were initially encouraged with the discovery that an episode of *theileriosis* in an animal led to immunity. Subsequently, however, distinct strains of *T. parva* were found, and it was established that broad protection could only follow exposure to a variety of strains. This obstacle was overcome when it was discovered that exposure to a combination of three different strains appeared to provide a broad immunity, and when it became possible to harvest sporozoite forms of the parasite from ticks. This became the basis of the live vaccine used in ITM. Different strains of sporozoites extracted from infected ticks were injected into animals, and the animals were simultaneously treated with long-acting tetracycline antibiotics. While preventing full-blown clinical manifestations, the immunised animals occasionally show mild and transient symptoms of the disease. ITM, although widely implemented, has several shortcomings. It requires a cold chain facility to maintain viable sporozoites, antibiotics, and expertise to monitor animals after treatment. These factors contribute to the high costs of ITM. Furthermore, immunised animals become carriers of the parasite, and can potentially infect ticks and spread *theileriosis*. ILRAD initially, and ILRI subsequently, have been involved in the quality control aspects of ITM stocks.

With regard to *theileriosis*, vaccine research remains ongoing, however. Certainly to an extent this is due to the fact that prospects for a vaccine against East Coast Fever are rather better than for *trypanosomiasis*, even despite the discovery of numerous strains of *T. parva*. Current vaccine research is organised in a very different manner than that of the *trypanosomiasis* vaccine efforts. The UK Department for International Development (DFID) are the primary funders of the research and due to their shifting funding priorities and prevailing donor thinking regarding the support of problem-oriented research they have played an important role in reorganising the way vaccine research is taking place at ILRI. It is certainly quite different and considerably more systematic than the original *trypanosomiasis* research at ILRAD.

DFID chose to fund the research from the position that the regional economy of East Africa loses about £300 million a year due to the disease (according to ILRI). Framing research funding from this perspective forces a shift towards a more systemic, problem-oriented mode of research that is built around the fact that the steps needed to make the vaccine work (and this means looking at the entire value chain, down to dissemination and appropriate costing) are as important as the continuing lab work. DFID, initially, did not view East Coast fever as a priority for intervention (Perry *et al.*, 2002) but nevertheless ILRI successfully built an argument for support of their research efforts. The proof of this was in the funding. The project, then, was funded on assessments of the likely impact of the vaccine on the economy and on livelihoods, the basis of the advanced state of ILRI's existing research and therefore the likelihood of a workable vaccine being developed, and the support of a system that would allow the vaccine to be effectively disseminated and widely used. The science was firmly framed within social and developmental contexts.

The project is housed at ILRI, but the systemic approach demanded by DFID means that a network of partners (not just lab partners) are essential to the success of the venture. The project is conceived of as a needs-based public-private partnership, ensuring all the components necessary to develop and disseminate the vaccine are in place in good time. A complex set of partnerships between the public and private sectors across two continents has played an important role in moving the science forward. Private sector ventures (the French biotech firm Merial) are involved in producing the vaccine for trial

and will be responsible for the delivery of the vaccine in a context where there is little demand, because of a lack of resources, and to pull the vaccine vials to where they are needed the most. Kenya's National Agricultural Research System plays a role in vaccine trials and monitoring, and further collaborators are based at universities worldwide.

There is a high degree of complementarity between the major partners. ILRI has conducted much of the essential immunological research. Kenya's state national agricultural research system is responsible for trialling the vaccine and monitoring impacts of cattle. Merial, a French biotechnology company, produce the vaccine. The modality of the project itself is shifting thinking about partnerships, about research, and about what constitutes excellence.

6 Implications for science policy: rethinking excellence

The ILRAD/ILRI experience clearly shows that something more systematic than excellent laboratory-based research was needed in producing concrete technologies to manage and control *trypanosomiasis*. Research 'excellence' reached a point where it became clear that the development of a vaccine was not going to prove possible despite 15 years of concerted effort. The tsetse fly continues to be a scourge but, as we have said, there are other methods of dealing with *trypanosomiasis* in cattle, for example the use of traps and targets and bush clearing. Much of the research for this had produced good quality science throughout Africa but ILRAD did not ever really engage with it as a 'research trajectory'. Several key players we interviewed underlined this as a 'missed opportunity' that had potentially delayed more effective vector management of *trypanosomiasis* by decades. Within ILRI itself science continues to play a role but in the more limited sense of diagnostic research and genetic characterisation of trypanotolerance in cattle. Of course the question still remains as to why these other research trajectories were not given more emphasis in the early days since despite the fact that ILRAD established itself as a centre of bovine immunology, arguably it could still have been proactive in other senses. ILRAD at that time, despite being a member of the CGIAR, had absolute authority to shape its own research agenda and we argue that it took a very narrow, institutional view of research excellence – it was conceived as an 'island of excellence' with little external engagement except with collaborating laboratories. This focus on excellence as science endeavour meant ILRAD did not seek to support a systemic approach that might have identified and supported other approaches. ILRAD's ability to broadly shape its own remit and research agenda led to a narrow conceptualisation of excellence. A tighter institutional focus on its own research agenda may have led to a more inclusive interpretation of excellence and consequently more engagement.

The case of East Coast fever is rather more complex. The ILRI developed ITM 'cocktail' was finally approved for limited use in Kenya in Maasai cattle in the last two years. This is the culmination of many years of research within ILRI/ILRAD and other East African research institutes. Other countries, particularly in Zambia with Belgian support, have continued to conduct live vaccine research, testing and production. In the intervening period live vaccines have been produced and used in Southern Africa. Vaccination has been adopted only slowly in Kenya (and this may have been due to vested interests on the part of either veterinary authorities and/or acaricide manufacturers). In fact it has been suggested that one reason for the recent relaxation of

prohibition in Kenya is the increasing problem of acaricide resistance in the tick population. Arguably this has made the need for alternative solutions much more pressing. A final argument concerns the high costs involved in production and delivery, though these seem nowadays to be smaller than they once were.

In fact ILRAD, in its initial mandate, made the strategic (and somewhat isolated) decision to look for an immunological solution to the East Coast fever problem. From that point on the organisation saw itself as a high quality centre for research in bovine immunology with the ultimate objective of discovering vaccines for both cattle diseases through molecular research into the biology of the problem. ITM research continued but gradually gave way to the alternative approach and currently it plays almost no part in ILRI activity. Hence it is apparent that there were (and still are) three prime mechanisms for dealing with the disease. In practice ILRAD took the view that the molecular East Coast fever vaccine should be the preferred route. This may have been in part due to problems associated with the complexity and the expense associated with the ITM method. At the time of the early decision in the 1980s it seems reasonable to suggest that existing organisations would have had trouble actually testing and delivering this crude vaccine no matter how successful it had been in early trials.¹ Conversely the prospects for a science-based vaccine must have seemed promising at the time and there was clearly excitement about being involved in such cutting-edge science.

However, the issue no longer seems so clear cut. The biology of *theileriosis* is now recognised as much more complex than initially expected and donors funding this research are inevitably wondering whether the expense will ever pay off in practical terms for the resource-poor farmer, no matter how good the research is in purely scientific terms.² There is some evidence (interview data) that the fallback remedy of cattle dipping is becoming less effective due to growing acaricide resistance in the ticks. In addition it may very well be that capacity now exists for ITM delivery because of institutional learning, improved infrastructure, donor support and possible private sector investment.

ILRI research is inevitably more embedded in a highly complex set of stakeholder interests than ILRAD ever had been. In the early days of the 1980s the organisation took the view that its major role was one of placing the bulk of its resources firmly behind the search for molecular vaccines. In a sense this became the central thrust, one that fitted well into a CGIAR ethos that focused on the great importance of strategic science and regional excellence in solving the world's food problems although why alternative scientific approaches were not given greater consideration at that time raises interesting questions of scientific management and political economy. Also though other stakeholder interests were present these appear to have played little or no role in such a specifically defined and science-led strategy. But as we have outlined the wider context has changed in several important ways. Nowadays, mainly as donor priorities have shifted, it is probably no longer possible for ILRI to function in an exclusively science-led modality. This leaves it in a position where it must rethink its research organisation.

One way of looking at this history is that it represents the shift of an institution operating according to Mode 1 principles to one operating on the basis of a Mode 2 agenda, which are explained later in the paper.

But the implications of the case are many. Reframing excellence is not a simple, notional process that equates to the binaries of 'Mode 1' and 'Mode 2'. Rather it occurs at the confluence of some of the most important and problematic issues of institutional

research and development and how it should be organised, how institutions internalise change, and how institutions engage within broader networks. It is a political, value-laden process. These values extend to other issues: if excellence is to be re-thought, how do we assess it? How do we define and audit something that by its design is meant to be fluid, innovative and systemic? Clearly there ought to be a move beyond just peer-review as a measure of success and progress, but what should it be? One of the issues of the DFID-funded *theileriosis* project has been how to assess progress. What the scientists view as progress and what DFID consider progress are not always the same, and simple peer-review does little to help.

There are more positive, less problematic implications. The process of broadly defining excellence is part of a process of understanding the need, and the reality, of a more systematic system of innovation perspective to shaping science and technology. There are many examples of the benefits of a system of innovation approach (Chataway, 2005; Hall, 2005) but one concern, raised above, has been how to operationalise something that by its own definition should be organic, complex and responsive. Perhaps new understandings of what 'excellence' is and how to promote it can lead to a more systemic, embedded science and technology in the context of developing countries? The final section reflects on some of the more theoretical dimensions of these questions.

7 Research excellence in context

Accounts of Mode 1/Mode 2 knowledge production literature describe a shift towards a process of knowledge production that is problem-oriented, multidisciplinary, contextualised and undertaken by teams of specialists from different fields and backgrounds (Gibbons *et al.*, 1994; 2003). This implies a change in the meaning of excellence, to incorporate objectives such as focused problem-solving. It also implies a change to the assessment of excellence, since the peer-review system may no longer be the dominant form of assessment; at a minimum it may require incorporation of measures of impact that consider output in terms of social outcomes and not only in terms of the production of applied research.

The innovation systems literature suggests that new challenges of S&T research can best be met by undertaking research within 'innovation networks'. These networks are defined by a web of collaborative relationships between organisations and their research partners, government and private funders as well as industry. Within this framework knowledge is produced as a result of applied research but also as a product of interaction between agencies. Again this implies a shift in the way research excellence is perceived since assessment must be concerned not only with evaluation of applied research (by peer review) but also with evaluation of the processes of institutional change that facilitate the production of the second type of (institutional) knowledge.

The importance of factors of institutional design is also identified in the discussion of the experience of interdisciplinary research (Scottish Higher Education Funding Council, 1997). This experience places emphasis on institutional design, not only in terms of physical location and organisational structure but also in terms of control pathways and resource allocation mechanisms. These elements are important because of their effect on the incentive structure of individuals, and also their influence on facilitating organisational change.

The concept of governance shifts the emphasis from institutions as physical structures to institutions as systems of norms and procedures. The emphasis here is on the importance of management structures, incorporating stakeholders, and systems of accountability for an organisation's capacity to produce excellent research. The implication for the assessment excellence from the discussion of both interdisciplinarity and governance is attention to institutional processes over and above institutional output (Shcolte, 2002).

Drawn together, the theoretical discussions potentially lead to two major conclusions for research excellence. First, research excellence may incorporate a broad set of objectives including scientific excellence but also social and economic impacts, the development of collaborative relationships and participative forms, good governance, effectiveness and cost efficiency. The second potential conclusion is that the expansion of outlook in terms of objectives will also require a corresponding expansion of outlook in terms of assessment, including output measures of research produced and the outcomes of this research, as well as analysis of institutional processes.

In response to the question of what constitutes research excellence, a manager of donor funds in this area stated: "In my opinion research that has an impact on the lives of people in developing countries is more excellent than excellent but is not usually described in terms of excellence". While this statement seems confusing at first, in fact it captures two important points relating to research excellence. First, talking about research excellence often involves people talking across each other – not only do people use different terminology to discuss excellence, they also mean different things when they use the term. Second, the statement effectively illustrates the difficulty of trying to bring together questions of relevance and scientific excellence in evaluation of research. The considerable difficulties of managing the tensions between different perspectives of excellence, of engaging in a dialogue on excellence that is substantive rather than just becoming part of development discourse, and of operationalising evaluation systems, should not be glossed over. Despite these difficulties the question of research excellence warrants serious consideration because of its profound importance. It is important, as a director of a major donor agency pointed out, for guaranteeing the credibility of research but also for achieving clarity on what is meant by research excellence and how it can be achieved is also key to ensuring that development aid for S&T research is managed in a consistent way and is as effective as possible.

An initial concern with the way S&T research is traditionally undertaken, and the corresponding definition of research excellence, is that it may not be sensitive to context and the need to promote development. Traditionally S&T research institutions have been organised around single scientific disciplines. Research under this approach is decided by a small group of scientists and aimed at disciplinary excellence and therefore may 'not give attention to whether an R&D institution has and properly articulates a sustainable development purpose'. In addition to the fact that such institutions are often treated as isolated entities rather than as parts of broader knowledge networks or innovation systems, this means that they are not responsive to specific social and economic problems and general national policy. As Dr. John Mugabe, Head of Science and Technology at NEPAD, articulates, development of such institutions has been very much 'supply-driven', with resources directed to infrastructure and training which 'left behind organisational entities that are not capable of responding to domestic and international demands' (Mugabe, 2003).

This underlines the question of whether current definitions of research excellence are even appropriate to developing countries. As Chataway *et al.* (2005) emphasise:

“Although institutions in industrially developed countries put much effort into judging and reviewing research performance, and can therefore legitimately claim excellence, it is not clear that institutions in developing countries benefit from using the same criteria.”

While it will always be necessary to have evaluation systems in place to ensure rational allocation of funding, it is important that innovation is not constrained by a particular understanding of what excellence might, or ought, to be.

Besides the need to promote development discussed above, there may be other objectives of S&T research in a development context that are not incorporated into definitions of research excellence such as international recognition and peer review. Ability to resolve specific development problems and generate appropriate technology towards sustainable development is, for example, an area that is largely neglected under this definition. Other concerns that are central to undertaking research in development S&T and therefore might also form part of a broader set of criteria, are cost effectiveness, community outreach, and capacity building.

Furthermore, there is a question of whether a measure of scientific productivity as the number of peer-reviewed journal articles provides the right incentives to scientists involved in development research. It is unfortunate for researchers in organisations in both developing and developed countries that current peer review mechanisms and research assessment exercises do not provide rewards for contributions made to development. As Maureen O’Neil, president of the International Development Research Centre (IDRC) underlines:

“Too often IDRC hears stories, especially from younger faculty, that they get little or no credit towards career promotion and tenure for the research they do on IDRC and CIDA-funded projects. This is considered ‘research for development’ or research that is worthy but not ‘excellent’.” (O’Neil, 2003)

The problems outlined above indicate the importance of re-considering research excellence – in itself and also in relation to development objectives. A final consideration is how research excellence relates to broader changes in the way science and technology research is undertaken. It may be necessary for the theory and policy design of research excellence to evolve in relation to new practice:

“Now is the time to consider how we define ‘excellence’ and ‘innovation’ and how we will measure research results against them. By ‘excellence’, we may mean ‘urgently needed and challenging research’ – that which is problem oriented, multi-disciplinary (preferably comparative) and carried out by teams networking internationally across research sites and policy jurisdictions. By ‘innovative’, we may value co-production of knowledge through innovations only made possible by bringing together the experience of experts in Canada and other countries and applying that knowledge to solve real problems.” (O’Neil, 2003)

It is clear to go beyond rethinking new forms of excellence to promoting and practicing new forms of excellence will require major shifts in the organisational and institutional architecture of R&D in developing country contexts. We are under no illusion as to the difficulty of affecting changes of this nature. The institutional history of livestock

research in East Africa and *trypanosomiasis* and East Coast fever research and development in particular presents a compelling case for both rethinking and reworking ‘excellence’, however.

8 Conclusion

It is our view that scientific research is clearly of fundamental importance in dealing with a variety of livestock diseases that continue to plague the poor farmer in this part of the world. And therefore it is also clear that research institutes like ILRI should continue to play a pivotal role in technology development particularly where the necessary interventions are science-based. It is also clear the CGIAR as a whole will continue to play a major role in defining and driving research for agricultural development.

We believe that more recent developments in ILRAD/ILRI’s history such as the merger, the failure to develop a *trypanosomiasis* vaccine, and shifts in donor priorities – one result of which is the DFID-funded *theileriosis* project, are incrementally broadening how ‘excellence’ is conceived. If excellence is conceived purely in terms of scientific research ILRAD had some initial successes in terms of widening our knowledge, if excellence is conceived from a developmental perspective (also part of its original remit) it could be argued it has failed in terms of providing solutions. Embedding the institution within a broader context and a broader set of stakeholders and networks inevitably leads to differing perspectives of what research ‘excellence’ ought to be. There are of course contestations between stakeholders and actors in view of what they may view excellence to be and managing this plurality will be one of the most important tasks facing ILRI and R&D institutions worldwide. Excellence has a political dimension as well.

We believe that this contestation can lead to the shaping of a broader, more-development focused and context-bound conceptualisation of what R&D ‘excellence’ ought to be in developing country contexts and this can in turn lead to better science policy and practice. Policy that understands the importance of systemic approaches, appropriate prioritisation of research, resources and activities, and demonstrates a more nuanced understanding of the contexts of need and of capacity which institutions, and research, operate. In this way science and technological practice can make its fullest contribution to development.

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Notes

- 1 At the time of setting up ILRAD, the countries of the region looked to it for the next generation of vaccines and, as molecular techniques for parasite characterisation were developed, they offered tools with which ITM strains could be better defined and compared. The cost of isolating, defining and producing national strains made it difficult for countries to adopt control measure and most national programmes have been sustained through donor support – UK in Kenya, FAO and the Netherlands in Tanzania now and Denmark in the past, and Belgium in Zambia.
- 2 Although there is evidence that the project is building other kinds of research and innovative capacity, cf. Smith (2005).